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THE CULMINATING POINT OF THE NORTH AMERICAN CONTINENT.¹

AMONG the objects for which the expedition recently organized under the auspices of the Academy of Natural Sciences of Philadelphia was despatched to Mexico was the determination of the physical features of the giant volcanoes of the South, with special reference to a study of the vertical distribution of animal and vegetable forms. While prosecuting our observations in this direction, I took the opportunity, in company with one or more of my associates, of scaling the four loftiest summits of the land; namely, the peak of Orizaba, Popocatepetl, Ixtaccihuatl, and the Nevado de Toluca. This gave me the advantage of making personal comparisons between the life that existed in different regions of "cloud-land," at the same time that it offered me the opportunity of more closely investigating the geological features of some of the most gigantic volcanic mountains known to us. Numerous measurements of altitude were made during the ascents, and, in the higher regions, always with the same instrument. This was a registered aneroid, tested and corrected at Philadelphia (imme-

diately before the starting, and shortly after the return of the expedition) at the sea-level of Vera Cruz, and in the Central Meteorological Observatory of the City of Mexico, at an elevation of 7,403 feet. To the officers of the latter institution I am indebted for the privilege of making comparisons with the standard mercurial column.

The results of our measurements show a striking accord in some instances with those obtained from earlier measurements, while in other cases they exhibit marked divergence. The fact that all the summits were ascended within a period of three weeks, were measured with the same instrument, and during a period of atmospheric equability and stability which is offered to an unusual degree by a tropical dry season, renders the possibility of errors of any magnitude almost *nil*. At any rate, such errors as may have crept in will probably not affect a general comparative result. The points of important difference are: (1) the highest summit of Mexico is not, as is commonly supposed, Popocatepetl, but the peak of Orizaba (Citlaltepetl, the "Star Mountain"), which rises 700 feet higher (18,200 feet); (2) Ixtaccihuatl, the familiar "White Woman" of the plain of Anahuac, is but a few hundred feet (about 550) lower than Popocatepetl.

The peak of Orizaba was ascended on the 6th and 7th of April, Popocatepetl on the 16th and 17th of the same month, the Nevado de Toluca on the 21st, and Ixtaccihuatl on the 26th and 27th.

The restoration of the peak of Orizaba to the first place among Mexican mountains, and its increased altitude, open up the interesting question as to what constitutes the culminating point of the North American continent. The only other mountain that need be considered in this connection is St. Elias, situated approximately on the 141st meridian of west longitude, and whose summit is claimed for both the possessions of Great Britain and the United States (Alaska). The measurements of this mountain depart so widely from one another, however, that we are not yet in a position to affirm, even within limits of a thousand feet or considerably more, how nearly it approaches in height the Mexican volcanoes. We are probably justified in dismissing without further examination the measurement made by La Pérouse in 1786, which gave for the peak less than 13,000 feet; and seemingly not much more reliable is the datum (14,970 feet) which appears in Capt. Denham's chart from 1853 to 1856, and is copied into the British Admiralty chart of 1872 (Humboldt's *Cosmos*, v. p. 419, Olté's edition; Dall, *Report of the United States Coast and Geodetic Survey* for 1875, p. 159). This latter figure (4,562 metres) is adopted by Petermann in his general map of North America prepared for Stieler's "Hand-Atlas" (1878-81). Malespina in 1791 determined the height, by means of angles taken from near the position of Port Mulgrave, to be 5,441 metres, or 17,851 feet; and the equivalent of this figure has been copied into the Russian hydrographic charts (1847). Tebenkoff reduces this amount by somewhat over 900 feet.

No carefully conducted measurements of the mountain appear to have been made between the date of the publication of Tebenkoff's chart (1849) and 1874, when Mr. Dall, under the direction of the United States Coast Survey, surveyed a considerable portion of the Alaskan region.¹ This investigator found four different values for the height of the mountain as measured from four points respectively 69, 127, 132, and 167 miles distant: these are 19,464, 18,350, 19,956, and 18,033 feet. Mr. Dall dismisses all of these as having little value, except the measurement of 19,464 feet, made from Port Mulgrave. It is difficult to reconcile the

¹ From the Proceedings of the Academy of Natural Sciences of Philadelphia.

¹ Mr. Dall, in his report above referred to (p. 159), quotes from Leopold von Buch an additional measurement of the mountain, namely, 16,758 feet. Grewingk (Verhandl. Russ.-Kaiser. Mineralg. Gesellsch., 1848-49 [1850], p. 99) gives the same figure, referring likewise to Buch (Canar. Inseln, p. 390); and a further reference appears in Davidson's Coast Pilot of Alaska, 1869, p. 142, note (16,754 feet, according to Grewingk). But this figure is manifestly Malespina's measurement given in French feet, which resolved is equal to 17,880 feet; and Grewingk himself quotes Malespina's measurement (5,441 metres) on p. 404 of his report. Humboldt (*op. cit.* v. p. 252) credits the measurement of 17,855 feet to Quadra and Galeano; but, as these observers were associated with Malespina, it is more than probable that the data here given are those which have been generally attributed to Malespina. Humboldt intimates that the measurement is perhaps one-fifteenth too great; but whether this assertion rests on certain facts contained in Malespina's manuscripts, which the great German traveller found among the Archives of Mexico (p. 419), or not, is not stated.

vast range of these measurements, whose extremes vary to an extent of upwards of 1,900 feet, or to one-tenth of the height of the entire mountain, except on the assumption that the angles of measurement were too small to permit of exactitude in the result. And, indeed, Mr. Dall himself rejects all his measurements except those made from Port Mulgrave, giving them "no weight in the result, as they were all taken at great distances from the peak, and subject to various disturbing influences and uncertainty in most of the positions" (p. 164). And yet it is upon the accurate determination of the position "At Sea," 127 miles distant, that "the position of Mount St. Elias depends" (p. 165); and necessarily upon the determination of this position must also depend the accuracy of the measurement of height. Malespina's measurement was made from a point apparently very close to that occupied by the Coast Survey officers, and his results, as has already been seen, vary negatively by 1,600 feet; but he estimated the distance separating him from the mountain at 55.1 nautical miles. Mr. Dall remarks, in relation to the discrepancy existing between the two measurements, that the doubt lies wholly with the distance. But this does not explain the great range in Mr. Dall's own results. And we are perhaps led to be the more suspicious regarding the value of these when we take into account the discrepancies which appear in the determination of the altitude of Mount Fairweather. Three series of sextant observations were made of this mountain from the region about Lituya Bay and Cape Spencer, with the result of obtaining an average value of 15,447 feet. Vertical circle measurements of the same mountain made from Port Mulgrave indicate 15,270 feet, while the average of all measurements is 15,423 feet. Mr. Dall calls attention to the close correspondence of these results, and comments more particularly upon the "unanimity in the Lituya Bay observations."¹ A reference to the exact results obtained, without recourse to the delusive system of extracting averages, shows, however, that in place of unanimity we have the reverse. Thus, the sextant observations taken from "Off Cape Spencer" indicate 16,009 feet; those from "Off Lituya Bay," 15,247 feet; and those from "Off Lituya," 15,085 feet (*op. cit.* p. 174),—a difference in extremes of upward of 900 feet. This divergence in the measurement of a mountain three miles (\pm) in height from positions twenty to fifty miles distant makes very doubtful the results obtained in the case of St. Elias, where the distances were still very much greater, and the angles of observation correspondingly smaller.

In view of the broad divergence existing in these later measurements, and the fact that all earlier determinations give less than 18,000 feet for the height of Mount St. Elias, geographers will probably consider the question of absolute height as still an open one. That the mountain closely approximates the giants of the Mexican plateau is almost certain; but it seems equally probable that its true position is after, and not before, the peak of Orizaba.

ANGELO HEILPRIN.

THINNESS VERSUS STOUTNESS.

THE following facts on the value of emaciation, from the *Lancet* of Sept. 27, are of interest. Emaciation is a prominent feature in many diseases. Many of the phenomena of disease are, in reality, efforts at repair. It will therefore be advantageous to inquire if some good purpose is not served by emaciation. To begin with diseases which affect the circulation, in many such we may note that the patient will rapidly lose flesh, and that when the loss has proceeded to a certain degree it is arrested. The patient becomes and continues thin. Not only is it difficult to fatten him, but he is not much benefited by the attempt. Should his disease be arrested, but leave some injury behind, the patient will probably continue thin. Instinctively, in consequence, we look for the presence of disease, active or quiescent, in sparsely developed persons; and practically we look for it in the territory of the circulation, either pulmonary or systemic, and generally find something. Such emaciation, ceasing at a certain point, does not much exceed what is seen in athletic training, and may be defined as an involuntary training, forced upon the patient by his ailment. What,

briefly, is the benefit of training? Unusual effort in untrained men or animals is checked, not by muscular exhaustion, but by congestion of the pulmonary circulation, and paralysis of the right ventricle of the heart. The effect of training is to enable the pulmonary circulation to keep pace with the increased activity of the systemic. Several changes concur to this end,—dilatation of the lung and of its blood-vessels, hypertrophy of the right side of the heart, and most especially absorption of every tissue which is found to be superfluous for the effort in view. This absorption reduces the area of the systemic circulation; and it is followed by a reduction in the quantity of blood, because a smaller quantity will suffice for active circulation through the lesser area. The individual so trained is in the condition of having a pulmonary territory larger than is necessary for his state of rest, and consequently with a greater margin for relief upon exertion. Conversely, the contrary condition of obesity develops the systemic circulation to the full capacity of the lungs, so that the least exertion will produce dyspnoea. Acute disease being like exertion, we can see the advantage of entering upon it in a state of training. The exhaustion to be feared is that of the heart's right ventricle; and the inconvenience of existing stimulants is, that they do not stimulate the right ventricle by itself.

To return to emaciation as produced by disease. Let us first take the case of diseases of the lung. Should the territory of the pulmonary circulation be diminished by such a disease as phthisis, it would be impossible for the patient, without emaciation, to have an active circulation in the systemic area without danger of congestion of the lung. The feeble attempts that are made at hypertrophy of the lung in this disease are thus met half way. The same thing is true of all emphysema of the lungs, whether senile or morbid. Then we may consider the disorders of the greater circulation. In disease of the valves, when the muscle has to do the work of the valves, and in decay of the aorta, when the ventricle has to combine the aorta's duty with its own, the smaller the volume of blood to be dealt with, the better. Without reduction of the area through which it flows, a reduced amount of blood would only result in a sluggish circulation. Emaciation in such cases is therefore salutary. Moreover, the capillaries are the seat of greatest friction: they are therefore the part to be reduced. This is the reason why all animals, in a state of nature, grow thin as they grow old. Man, and the creatures under his control, may violate this law, but not with impunity. Acute disease, when the arteries are decayed, is doubly dangerous. The right ventricle of the heart may be exhausted, as before, but now the left ventricle may also prove unequal. Stimulants here can have but little drawback; but in surplus lung, and an amount of blood that is well within the powers of propulsion, lies a greater safety. Obesity is dangerous to the aged.

It would be well to consider the phenomena of waste in fever by the light of an assumption that they are salutary. The brute creation, when suffering from fever, eat nothing; yet they do not die, but recover. The body seems destined to feed upon itself, and to delay all repair until convalescence. Great responsibility rests upon those who supersede this instinct by an artificial method. Graves, who fed fevers, at the same time bled his patients. Venesection has this drawback, that to relieve the right heart we are obliged to stint the left, and to reduce the systemic circulation to a sluggish flow. The speedy repair of such loss of blood shows that its benefit must be only temporary. Such a remedy is an imperfect substitute for a capacious lung. The perception of this inconvenience has led to the disuse of bleeding. The concomitant pressing of food in fevers should probably follow it. The most evident of natural remedies for any kind of fever are starvation and emaciation. However indulgent appetite may be in health, it returns in disease to the strictest authority. Obedience to its dictates may shatter superstitions, but will not jeopardize the cure. In chronic diseases, the physician who thinks of the future of his patient will look grudgingly on fat. It provides for warmth and irregularity of nutrition. If these be provided for in other ways, much relief can be given to the circulation; and if in acute disease the presence of a tissue should prove an embarrassment, the physician will not interfere with its removal.

¹ Including here the measurements made off Cape Spencer.